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AN ESSAY ON AUSCULTATION AS APPLIED TO CARDIAC DISEASES,

CONTAINING A NEW HYPOTHESIS REGARDING THE SOUNDS OF THE HEART.

BY CHARLES HOOKER, M.D.

[Communicated for the Boston Medical and Surgical Journal.—Continued from p. 300.]

3. THIS *hypothesis rationally explains the phenomena produced by disorders of the heart, and especially those produced by derangement of the valves.*

In treating of the different diseases of the heart, the various pathologic phenomena will be particularly described and explained. An explanation of these phenomena in this place, would, therefore, only occasion unnecessary repetition.

Some general observations, however, will be premised under the two following heads: 1. The variations of the impulse and sounds occasioned by hypertrophy and dilatation of the heart; and, 2. The morbid murmurs, and the purring tremor of the heart and arteries.

1. *The variations of the impulse and sounds occasioned by hypertrophy and dilatation of the heart.*

(A) *In hypertrophy* the parietes of the heart become increased in thickness, from the acquisition of a preternatural quantity of muscular substance. Supposing each ventricle to be one single muscle—this muscle, in hypertrophy, becomes a larger, thicker, and, consequently, a stronger muscle. Hence the contraction of the ventricle is stronger than natural, and the *heaving motion* of the impulse more powerful—its power being, as Laennec observes, "directly as the thickness of the walls of the ventricles." But, in accordance with a general law of muscular action, that the power of action is inversely as the quickness or rapidity of action—the power being proportioned to the size, and the rapidity to the length of the muscle—indeed, in accordance with the general law of the mechanical powers, that what is gained in power is lost in velocity—the contractions of the hypertrophied heart, though stronger, are slower than natural, and the heaving motion of the impulse is more prolonged. "The more intense the hypertrophy," says Laennec, "the longer time the impulse is perceptible."

With the slow motions of the hypertrophied ventricle, it is obvious that the occlusion of the cardiac valves must be less sudden, and the arrest of the refluent blood, at the orifices closed by the valves, less abrupt, than natural. Hence, it is rationally explained, why in hypertrophy the strong heaving motion of the impulse is attended with little or no *vibratory succussion*,* and the *clacking sounds* of the heart are dull. As Laennec says, "In moderate hypertrophy the contraction of the ventricles produces only a stifled sound, analogous to the murmur of inspiration, and the clack *(claquement)* of the auricles [the second sound] is much less sonorous than in the natural state. In hypertrophy carried to an extreme degree, the contraction of the ventricles produces nothing but an impulse without sound, and the sound of the auricle [second sound], become extremely dull, is scarcely audible."

(B) *In dilatation*, the variations of the impulse and sounds are directly opposite to those produced by hypertrophy. The dilated ventricle, if considered as one single muscle, has become a longer and thinner muscle. Its actions, therefore, in accordance with the above-mentioned general law, are quicker, though weaker, than natural. The *heaving motion* of the impulse is, therefore, merely a sudden feeble blow, and in great dilatation it is imperceptible. But the quick, rapid motions of the dilated ventricle must accelerate the sudden occlusion of the cardiac valves, and of course render more abrupt the arrest of the refluent blood at the orifices closed by the valves—hence, the *vibratory succussion* becomes the prominent portion of the impulse, and the *sounds* are "louder, shorter, and clearer than natural." Dr. Hope observes, that "By dilatation, the impulse is diminished, often to the extent of being imperceptible. When perceptible, it is a sudden brief blow, which communicates a shock or vibration to the thoracic walls, but has not power or duration to elevate them.—The first sound in dilatation, becomes loud, brief, and clear, like the second.—The second sound also, is increased."†

(C) *Hypertrophy with dilatation* conjoined, occasion variations of the impulse and sounds, which are compounds of those produced by the two affections singly. The ventricle becomes a strong muscle, from the increased thickness and breadth of its muscular parietes; and the increased length of its muscular fibres, gives rapidity to its motions—its motions, therefore, are both strong and rapid. The contraction of the ventricles, consequently, produces a powerful heaving impulse; and the sudden and violent occlusion of the valves occasions strong vibratory succussions, and very loud sounds. The enlarged heart is, from its size, almost constantly crowded against the thoracic parietes, so that the sounds and vibratory succussions are the more readily propagated to the surface. The second sound is attended with a distinct vibratory succussion, or, as Dr. Hope terms it, a *back-stroke*, which he considers as particularly diagnostic of this affection. In this affection, Laennec says, the ventricular contractions are very perceptible to the hand, "which is forcibly raised by the sharp, dead, and violent pulsations;" and "we frequently perceive the head, limbs, and even the bed-clothes, strongly shaken at each contraction of the heart."

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* That is, the vibratory succussion produced by the internal action of the heart, which I attribute to the valves. The impinging of the heart against the ribs, in hypertrophy, frequently causes a strong vibratory shock.

† The increased loudness and clearness of the second sound, in dilatation, seem altogether inexplicable according to Dr. Hope's hypothesis—at least his explication is unsatisfactory. His explication of the *natural* second sound is as follows. When the diastole of the ventricle takes place, the blood "shoots with instantaneous velocity from the auricles into the ventricles; and the reaction of the ventricular walls on its particles, when their course is abruptly arrested by the completion of the diastole, is, I conceive, the cause of the loud, brief, and clear sound." (Hope's Treatise, p. 49.) In dilatation, he says, the second sound is increased, "because the quantity of blood entering the ventricles during their diastole is probably augmented, and, from the thinness of their walls, the check is more sudden." (p. 53.) But if, as he suggests, the quantity of blood entering the ventricles during their diastole is augmented, an augmented quantity of blood must be sent into the arteries by the ventricular systole, and, of course, an augmented quantity of blood must circulate through the whole arterial system. All the symptoms of dilatation, however, indicate a diminished quantity of the circulating fluid. Dr. Hope himself states, that "the effect of dilatation is to enfeeble the heart"—causing "palpitations of a feeble, oppressed kind." "The pulse is soft and feeble, and, if the debility of the heart be considerable, it is small." And, "The languor of the arterial circulation causes the extremities and surface to be chilly," &c. (p. 236.) The other part of his explication is equally unsatisfactory—"from the thinness of their [the ventricular] walls, the check is more sudden." How can we conceive that the thin, feeble, and flaccid walls of a dilated ventricle, would cause a more sudden and sonorous check to the influx of blood, than the thick, firm, and rigid walls of the healthy ventricle?

From several passages relating to this affection, in the treatise of Laennec, it would appear that he had some indefinite apprehension of the distinction, which I have made, between the *heaving motion* and the *vibratory succussion* of the impulse—though he had no apprehension of the latter, as caused by some internal action of the heart. "There is one case," he says, "in which we are able in some degree to distinguish the shock communicated to the walls of the chest, from that conveyed to the ear. This is the complex case of hypertrophy and dilatation of the ventricles, but with the latter affection more marked than the former. In cases of this kind the impulse is usually not great, except during the existence of palpitation; and it has a very different character from that produced by simple hypertrophy: the beat of the heart is hard, with a sound like that produced by the blow of a mallet; but the blow seems confined to a small space; it is expended on the walls of the chest, and does not communicate to the ears an elevation or upward pressure proportioned to its force: it differs from the impulse occasioned by a strong hypertrophy in this, that, in the latter case, the distended ventricles appear to come in contact, in their whole length, with the walls of the chest, which yield before their pressure; whilst in the other case, the mere point of the heart seems to strike the thoracic parietes, with a sharp definite blow, which produces in these rather a vibration, than an actual elevation." (Forbes's Laennec, Ed. 1828, p. 554.) Again, in treating of this affection (p. 609), he says, "Sometimes, during these palpitations, besides the impulse of the heart which seems communicated by a large surface, we can distinguish another, which is sharper, clearer, and shorter, although occurring at the same time, and which seems to strike the walls of the chest with a much smaller surface."

The modifications of the sounds, which I have described, are such as commonly attend hypertrophy with dilatation—at least when the degree of dilatation is moderate. In a large proportion of cases, however, of *great* dilatation, either with or without hypertrophy, the clack of the first sound entirely disappears, being replaced by a *bellows-murmur*—a prolonged blowing, hissing, or whizzing sound, to be hereafter noticed. This murmur, in such cases, is probably occasioned by a regurgitation of blood from the ventricles into the auricles, during the ventricular systole—owing to a disproportion between the morbidly dilated auriculo-ventricular orifices, and the valves designed to close them. In some cases the second sound is, in like manner, replaced by a bellows-murmur—more seldom, however; for the arterial orifices do not so commonly partake of the general dilatation, with which the heart is affected.

(D) *General Conclusions.*—From the preceding remarks, it will be observed, that the heaving motion of the impulse is occasioned by the strength of the heart's action—depending, principally, on the thickness of the ventricular parietes.

On the contrary, the vibratory succussions and the sounds (produced by the occlusion of the cardiac valves) are occasioned by the rapidity of the heart's action—depending, principally, on the expansion of the ventricular parietes, or the length of the muscular fibres constituting these parietes.

It is obvious, however, that, in the former case, rapidity conjoined with strength of action, must cause the heart to impinge against the thoracic parietes with greater force—rendering the heaving motion more sudden and violent. So, in the latter case, strength conjoined with rapidity of action, by occasioning the occlusion of the valves to be both sudden and forcible, must augment the vibratory succussions and sounds.

Similar modifications of these phenomena may be produced, also, by mere functional derangement of the heart's action, when the structure of the organ is natural. In cases of apoplexy, in which there is a general prostration of the nervous system—and ordinarily when the heart loses its accustomed irritability—a slow, heaving impulse, an absence of the vibratory succussions, and a dullness of the sounds, are observed, like what occur in hypertrophy; the impulse in these cases, however, has not the *strength* of hypertrophy. So, in cases of a high degree of nervous excitability, as in hysterical subjects—and, in general, when there is excessive irritability of the heart—the abrupt impulse, the suddden vibratory succussions, and the short, clear, and loud sounds, much resemble those of dilatation, or dilatation with hypertrophy. From these circumstances, mistakes in diagnosis might be frequently incurred, unless the existing excitability and excitement, and the condition of the system generally, are carefully considered.

[To be continued.]

ARDENT SPIRIT.

Observations on the Use of Ardent Spirit "as a Medicine." By J. A. Allen, M.D., of Middlebury, Vt.

[Communicated for the Boston Medical and Surgical Journal.]

The object for which distilled or ardent spirit has been usually administered in disease, is to produce excitement or stimulation. This it obviously does; and like its kindred narcotics, opium, tobacco, stramonium, &c. it is always succeeded by narcosis, depression or torpor, which in degree are in proportion to the primary exaltation. As ardent spirit produces a greater excitement than most other narcotics, consequently the subsequent exhaustion is more striking, and like each of its associate narcotics, it is *sui generis*. It is this *exhaustion* to which I wish more particularly to direct medical attention.

This subsequent exhaustion or prostration, which is as inseparably connected with the operation of ardent spirit, or, indeed, constitutes a part of the effects of an alcoholic potation, as much as the incipient sti-

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DEBATE IN THE LONDON MEDICAL SOCIETY ON THE FACTS RESPECTING RICE, DISCLOSED BY DR. TYTLER.

"**Dr. Whiting.**—The Society, Sir, is never better employed than in receiving and attending to facts connected with medicine, and especially when relating to the disease now under its consideration. We are deeply indebted to this gentleman for laying these statements before the Society, and, through the Society, I presume, before the profession. We are not, it is true, prepared to say that the consumption of rice has been the cause of cholera in this country, for we are as yet incapable of pronouncing a decided opinion on the subject, our attention having never before been called to it; but now that these facts are before us, we can endeavor, should the unhappy necessity, which Heaven forbid, again occur in this country, to ascertain what share rice has in its production. On looking back, however, I do not remember a single case here in which it has been said or surmised that rice was eaten prior to the appearance of the disease. Rice, certainly, may not have been thought of, for we are accustomed to regard it as a harmless grain, but I think I know some instances in which no rice had been used. Yet it *may* have been. On a future day, however, some certain conclusion may be arrived at on this highly important question.

Dr. Johnson.—Not having been present last evening, I do not know that I ought to make any remarks on what has fallen to-night from Dr. Tytler, as he may on the former occasion have used many ingenious arguments in support of his views, of which I am ignorant. But I should like to know how he explains the facts of cholera having run all along one bank of a river, and not touched the inhabitants of the other—of its having committed extensive ravages in one wing of an army, and left the other free—of its having severely raged on one side of a street, and not on the other—the people in each instance all using the same kind of diet. These things are very difficult to reconcile with Dr. Tytler's statements. When the doctrine of pernicious rice was broached and strongly maintained, by the able and acute gentleman before us, in India, it made no kind of impression there. Indeed, I do not recollect one single convert to his views. In the three medical reports from the three Presidencies, nothing whatever was said of them; they were merely alluded to as peculiar opinions. How strange, also, it is that amid all the free trading with England, it should not have broken out here before 1831, and that even then it should first have appeared in Sunderland—a town which has very little direct communication with the East—and should have spared London for five or six months! Moreover, cholera has prevailed in many places where rice has scarcely even been known. I myself saw cases of cholera in huts in the Highlands of Scotland, the inhabitants of which would not have known rice had they seen it, and where the families lived almost entirely on oatmeal. The disease behaved with the same rudeness in Ireland, where they have only potatoes and buttermilk. It crossed over to America, and raged far worse there than it did here.

Dr. Tytler will say, 'It was because they ate rice there;' but I reply, 'They have no occasion to use a grain of bad rice in America, for it is well known that the Carolina rice is the very best in the world.' In short, Sir, I have not this night heard a single fact which induces me to believe that rice produces cholera; but, on the contrary, I have as good reasoning as Dr. Tytler for saying that cholera produces rice, in proof of which I might refer to the rice-water evacuations which notoriously result from the disease. (*Laughter around.*)

Dr. Tytler.—May I answer that gentleman? In the first place, then, I will reply to his question about its appearing in some wings of a regiment, and not in another, when marching, by reading the following extract from the *Calcutta Journal* of October 7, 1819:—'A part of the Bengal force, with some of the Madras troops, were detached to Canada, when, on their return, they discovered that ottah was not procurable; consequently they lived for about six days almost entirely upon rice. The first march they made on their return, the cholera appeared in the detachment, and continued to rage with great fury for ten days afterwards.' And this was written by an adversary, who, struck with the force of the fact, told me to make the most of it. As to his objection respecting the two sides of a street, why the fact is, that in such instances the people on one side had eaten the diseased rice, and those on the other had not. (*Laughter.*) The fact is, Sir, in short, that to the great influence which Dr. Johnson's writings have had in India is owing much of the circumstance that the extraordinary facts which I have now laid before this Society, maintained no footing there after he had pronounced an opinion against them in his journal. When Dr. Johnson received my statements and papers, he wrote in his 'Medico-Chirurgical Review,' for October, 1818—'Dr. Tytler here and throughout the paper labors to trace the cause of this epidemic to damaged rice; but the reasonings are so bad and the facts so dubious, that we entirely omit them.' I say that Dr. Johnson's authority is nearly predominant there, and he pronounced against my statements. Hence to Dr. Johnson, and to Dr. Johnson only (*with great warmth*), do I, in a great measure, ascribe the spread of the cholera subsequent to my discoveries. Then as to the rice-water evacuations showing that 'cholera produces rice,' why, ah! ah! I might say, how came rice-water evacuations to exist at all if no rice was taken? I wished, gentlemen, to refer as little as possible to official reports during this investigation, but as Dr. Johnson has quoted them in the Madras and Bengal reports, I must do so. In the first place Dr. Johnson will find, in the Madras Medical Board Report, that my opinion is alluded to; and in the Bengal Report, it is stated, that 'much bad rice grew in 1817;' and in the introduction to that Report, is mentioned the irruption of a disease which destroyed Col. Pearse's detachment in 1781. From the statement given in the Medical Board's Report, there is no question that that disease was the same as that which appeared at Jessore in 1817. Yet, in the Report, no mention is made of bad rice being eaten, although it has since come out by means of the *Indian Military Repository*—a work, in the publication of which I had no concern—that Col. Pearse, in 1781, actually complained officially, that the red rice which was then supplied to his troops was 'of a very inferior and prejudicial quality, and produced violent pains in their bowels, and fluxes.' It is not necessary that rice should be eaten in the shape of *grain*. The rice is eaten in all shapes. It is well bleached, and comes (amongst other forms) in that of flour, which cannot be distinguished from wheat flour, ful, it will arouse the latent energy of the system, and it is sufficient time to stop administering the spirit when the system begins to respond to its influence. This is a fallacious and dangerous doctrine, which has cost many unfortunate patients their lives. There are numerous well-authenticated instances of persons who for a wager, or some other frivolous cause, have quaffed a large quantity of distilled spirit at a time, and paid for their temerity with the sudden loss of their lives.* "No one," says Dr. Paris, "will deny the stimulating power of alcohol; and yet a large draught of this liquid will occasion extreme exhaustion without the occurrence of any signs of previous excitement." Brodie, by the injection of alcohol into the stomach of animals, caused death so instantaneously, as to preclude all probability of its having been absorbed, or of its inducing excitement; and a stout rabbit to which he gave two ounces of proof spirit, was in a short time killed.

These facts, it would seem, are sufficient to show the danger of giving ardent spirit to persons in a state of sedation, prostration or exhaustion, in large quantity. In fine, there is, it must be admitted, much truth in the assertion of an eminent writer on Materia Medica, that "distilled spirits are preparations of no great importance." "Like the distilled waters, they serve merely as vehicles for the administration of more active medicines." As a pharmaceutic agent, proof spirit or alcohol is more important than as a medicine; but as a pharmaceutic agent it has obviously been too incautiously and inconsiderately introduced, in some cases, even in violation of some of the first rules of Pharmacy, and to the injury of some medicinal substances with which it has been united. Some instances of this nature may form a subject for future communications for this Journal.

CASE OF CYNANCHE MALIGNA SIMULATING CROUP.

To the Editor of the Boston Medical and Surgical Journal.

SIR,—The following communication is at your disposal.

Yours, respectfully, CHARLES MACOMBER.

Marshfield, Dec. 24th, 1833.

Nov. 11th, 1833.—C. W., son of C. W. M., aged 3 years 11 months and 8 days, constitutionally unhealthy, this morning gave signs of indisposition. At evening was evidently feverish, and had an emetic of ipecac. which was followed by three ejections.

12th.—Was still indisposed. On examination, the fauces were found to be red. Had sub. mur. hydrarg. which produced several copious dejections. Had a spoonful of capsicum tea, secundis horis.

13th.—Ash-colored spots appeared in the fauces. Had an emetic of ipecac. and sub. mur. hydrarg.; and had sub. mur. hydrarg. 4tis horis in such doses as to move the bowels slightly during Nov. 14th, 15th, and 16th. During this time the disease was running its course much in the usual manner. Evening of the 16th had some cough and hoarseness. Took ipecac. and sub. mur. hydrarg. which operated, but without apparent benefit. At 3 o'clock in the morning, Nov. 17th, the ulcers in the throat had evidently extended to the glottis, inducing spasm and a train of symptoms exceedingly distressing, simulating those which occur in the worst cases of cynanche trachealis. In the struggle for breath the patient seemed to spring nearly a foot from the bed. Fearing immediate suffocation, gave patient gtt. 10 tinct. opii, with gr. 3 sub. mur. hydrarg.; spreading a folded strip of flannel with ung. hydrarg. fort., applied it to the throat, and, watching the movements of the patient, followed this dose with doses of gtt. 6 tinct. opii, together with small doses of sub. mur. hydrarg. at very short intervals, till the breathing became in some degree improved. Before 11 o'clock in the morning gave several emetics of pulv. ipecac. and pulv. sulph. zinci, āā ℈j. Pulv. scill. marit. and pulv. sanguinar. canad. āā gr. 6. About 10 o'clock one of these caused the ejection of a quantity of consolidated mucus, which sunk in water.

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* Some years since, a lady *in delicate circumstances*, in a mountain town in Vermont, had *a longing*, as it was called, *for* brandy; and as the kind matrons said no injury would occur from the indulgence of her appetite to satiety under such circumstances, she drank a pint of the desired liquid, and in a short time *she was a corpse!*

At 11 o'clock in the morning, eight hours after symptoms threatened suffocation, the little patient had taken upward of gtt. 70 tinct. opii, breathed much more easily, was a little drowsy at intervals, but still was wakeful more than three-fourths of the time. Fearing that the patient might get too much from under the influence of the remedy, continued for a time to give gtt. 6 tinct. opii secundis horis, then same dose 4tis horis, then same dose 6tis horis, and then the same dose 8vis horis. After the system had become fully impressed by opium, the patient was allowed to take cold water at pleasure, which it was disposed to swallow in considerable quantities; but more of the cold water it was inclined merely to draw into the mouth from one vessel, immediately ejecting it from the mouth into another vessel held to receive it. At intervals it would continue this operation for many minutes together. This was doubtless nature's impulse. The cold water was evidently a most excellent mouth-wash.

On the morning of the 9th day of the disease the patient desired warm tea, which it drank in large quantities, and which at length induced puking, followed by the ejection of a quantity of sloughs from the throat. Nov. 20th, the sloughs, as far as could be seen, had been thrown off, and the fauces appeared natural, except that in many places they were deeply indented.

It is concluded, that when cycanche maligna is followed by spasm at the glottis, there is a call for energetic practice. If temerity has slain its thousands, timidity has certainly destroyed its tens of thousands. In cases like this, a good rule is to look at the patient and observe the effects produced by the opium, and not to look at the amount of the remedy already administered. The eye of the experienced physician will, from the appearance of the patient, readily determine the dose that is required.

In cases where large doses are requisite, it will be necessary to lessen those doses in a gradual manner; otherwise an alarming prostration of strength may be the result. In the above case the gradually diminished doses of opium seemed to operate as a cordial, and were followed by an increase of appetite, and the little patient recovered without the use of tonics. Under some circumstances opium is a better cordial than wine.

In cases of pure croup, i. e. in cases where there is no ulceration and nothing of cynanche maligna, the treatment by frequent exhibition of small doses of sub. mur. hydrarg., as directed in a communication made by me to the Mass. Med. Society, has in my practice been uninterruptedly successful; but in cases of cynanche maligna simulating croup, which have often been called croup, although improperly, as well as in all other cases of ulceration in the region of the glottis inducing spasm, mercurial preparations alone are insufficient remedies. In these cases, as well as in all doubtful cases where the disease does not readily yield to the mercurial practice, opium should always be tried, and should be administered in frequent and sufficient doses to meet the exigences of the particular case.

CASES OF NEURALGIA, WITH REMARKS.

BY W. A. GILLESPIE, M.D. OF LOUISA COUNTY, VIRGINIA.

[Communicated for the Boston Medical and Surgical Journal.]

It is not perhaps sufficiently well known to the members of the medical profession that toothache, and what is vulgarly called jawache, are frequently of neuralgic character and of miasmatic origin.*

Many cases of this kind are submitted to the manual dexterity of some neighboring mechanic, who extracts tooth after tooth, until at length the disconsolate sufferer, without experiencing the least relief, frequently abandons himself, in despair, to the agony of the most excruciating tortures. Several cases have come under my observation, in which perfectly sound teeth have been removed, and often several in succession, in consequence of no relief being afforded from the extraction of the first. In addition to this, *nostrum* after *nostrum* is tried in vain; toothache drops of various kinds, many of which contain mineral acids, the tormented sufferer uses in profusion, to the eventual destruction of his few remaining instruments of mastication, credulously reposing confidence in the assertion of the boasting empiric, who vends them as *perfectly innocent!*

I am, by no means, opposed to the extraction of teeth, in proper cases, with due discrimination and reference to collateral circumstances. Carious, useless teeth should always be removed; but under few circumstances would I be disposed to remove a sound, useful tooth. When odontalgia is produced by slight caries, complete success has been known to follow the mere starting of the tooth from the socket by the instrument, and immediately pushing it back into its natural position; thus destroying its nervous connection whilst it adhered firmly to the socket, and causing it to answer, in a good degree, the purpose for which it was intended.

The following cases exemplify the success of quinine in neuralgic odontalgia. Notwithstanding the labors of many late writers, the pathology of neuralgia is still involved in much obscurity; but I believe no remedy, as a general one, is preferable to the sulphate of quinine. Many cases that were formerly termed rheumatism, ought manifestly to be classed with neuralgia; it is in this way, perhaps, we can account for the many high encomiums passed on the Peruvian bark as a remedy in acute rheumatism, by Drs. Hugh Smith, George Fordyce, Haygarth, &c.

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* M'Culloch, Bell, etc.

Case 1.—July 29. Mrs. S. had been subject to violent toothache, or jawache as she termed it, for three months. During this time she had applied to a mechanic, who extracted, at different periods, three sound teeth. No relief being afforded, it was thought proper to have professional aid; and accordingly I was called to the patient a few hours after the extraction of the third tooth. It was one of the large molares of the upper jaw, perfectly sound, and its removal had had no effect on the pain, which was of that lancinating, shooting character, too intolerable almost to be borne, at least not without the greatest fortitude. Upon inquiry, finding the pain to have regular periodical exacerbations, and knowing the patient to be exposed to the miasms of a marsh, which had produced, in the same season, some violent cases of remittent and intermittent fever, I viewed the disease as of miasmatic origin, and prescribed sulphate of quinine in free doses. There was in a short time a suspension of the paroxysms, and the patient, on account of the disagreeable taste of the quinine, which was given in solution, discontinued its use.

August 15.—The paroxysms of pain had now returned more severe than usual. I was again called to the patient, and again prescribed quinine, which was not taken as directed, and on the 20th August I saw her again. I now prepared some pills of quinine, and represented to the patient the absolute necessity of taking them constantly and regularly, and predicted with confidence almost certain relief from the most excruciating torments. She now took the medicine as prescribed, regularly, and had the satisfaction of informing me during the same week that she had escaped the periodical returns of her distressing complaint, and was now well satisfied that the medicine had controlled it. She has since had no return of it.

Case 2.—September 28, I was called to Mrs. K., residing in the immediate vicinity of the foregoing case, and exposed to the same miasms. She had suffered for ten months with violent toothache, as she termed it, and had had two teeth extracted, both perfectly sound, with no relief; and having heard of the preceding case, she concluded that hers was a similar one, which I found on examination to be nearly so, the paroxysms, however, being more irregular in this than in the former case. Quinine was prescribed as above, and in a few days entire and perfect relief followed. Fomentations, blisters and purgatives, had been previously used in this case, with no perceptible permanent relief. She has since had no return.

Ellisville, Dec. 15, 1833.

mulation does, it would seem is in a great measure overlooked by many practitioners. This circumstance evidently constitutes a cause of many of the *uncommonly sinking complaints* about which we have, for the last ten or fifteen years, heard so much, and which has so strenuously been attributed to a change in the *prevailing diathesis*. That a change in the diathesis has occurred within the last twenty years, I think no intelligent physician, who has been in practice that length of time, will question; and, that the free use of alcoholic narcotics, which this change in the diathesis has called into use, has in numerous instances contributed to augment the sedation or depression to an alarming extent, is, in my apprehension, equally certain. I could illustrate the truth of this position by numerous instances, directly in point, which have fallen under my immediate observation. One or two examples will, however, suffice.

In accordance with the advice of an esteemed medical friend, I commenced the use of *brandy*, in doses of half a tablespoonful every two hours, with a patient of mine, to relieve a distressed sinking at the pit of the stomach. At first, the gastric sinking was temporarily relieved, but soon recurred; and to obviate its recurrence, the brandy at each repetition had to be augmented in quantity. This course was continued for a fortnight, when a pint of good brandy a day was insufficient to afford relief to my suffering patient. Indeed, such was her "gone feeling," as she expressed it, that she was almost constantly calling for the brandy. No symptom of intoxication appeared, and no improvement in her health was to be discovered. The potations of brandy were discontinued, and stimulants and permanent tonics substituted. The distressing sinking gradually abated, and my patient eventually got well without suffering any serious inconvenience, more than having been kept sick two weeks longer than she would have been under a more rational and pathological mode of treatment.

Another lady of a delicate constitution and of a nervous temperament, to whom I was called in consultation, was supposed to have a *sinking typhous fever*; and *to obviate the sinking*, she had, for a week, under the guidance of her physician, who by the way is an excellent man of the temperance class, *taken daily a quart of French brandy*, considerable tincture of cantharides, capsicum, &c.—and in lieu of being satisfied with this liberal potation, she was constantly calling for more to relieve her from the indescribable gastric sinking she experienced. The suspension of the alcoholic narcotic demonstrated the fallacy of the method of practice adopted. From her supposed state of collapse, or exhaustion, she gradually recovered by the use of a few simple stimulants and tonics. Not, however, to her wonted state of health; for the system had sustained such a derangement, especially the nervous system, from the *deleterious narcotic*, that sound health will not by her be again enjoyed.

The functional and organic derangement produced is the same, whether ardent spirit be administered medicinally or as a beverage, when taken in any quantity. The design for which it is given does not change its nature. Whether in health or disease, taken in any quantity, it first excites, then depresses, the vital energy. This latter property greatly diminishes its value as a stimulant. It renders it uncertain and unsafe—causing, in fact, the very difficulty it was designed to relieve. As a stimulant, it is certainly less safe and less useful than the ordinary stimulants, such as the essential oils, ammonia, capsicum, piperine, tincture of cantharides, &c., because these are not subsequently, as a part of their operation, followed by exhaustion, or any kind of narcosis. They may be repeated without any danger of producing torpor or insensibility. If they be administered in too excessive quantity, their effects will be shown by augmented action or some obvious local irritation, or disturbed function, which will be unequivocal. The effects of the medicine and the original disease can easily be distinguished. Ardent spirit, unlike the common stimulants, if the quantity taken be considerable, quickly excites the force of the circulation and all the functions, and is speedily followed by temporary delirium and torpor; and in a still larger dose it occasions death, without scarcely any symptom of previous excitement. Indeed, it is well remarked by Dr. Murray, that "the stimulant operation, however, is not sufficiently permanent or capable of being regulated, so as to avoid the injurious consequences they are liable to produce, to admit of their being employed, except as occasional remedies." Except as occasional remedies; that is, when better remedies are not at hand, or cannot be obtained. Spirituous liquors, therefore, are only to be used when other articles cannot be obtained. They may then serve as a *temporary substitute;* just in the same point of view that *horse flesh* served the French soldiers as a substitute for *common beef* in their forlorn retreat from Moscow. Upon this principle, I once saved the life of a valuable young lady, whom I found in a state of exhaustion, apparently in the agony of death, by feeding her with warm sling of whiskey sweetened with maple sugar. As soon as they could be obtained, she was put upon the use of more salutary stimulants, and health was the happy result.

In cases analogous to this, ardent spirit may, for a short time, supply the place of better medicines. It may also be sometimes, under certain circumstances, advantageously given for the purpose of producing relaxation or prostration. I have been informed of one or two instances of the successful administration of ardent spirit to produce muscular relaxation to facilitate the reduction of a dislocated limb. It is obvious, from the relaxation we observe in the muscles of drunken persons, that during such a period a misplaced limb might be reduced. Since, however, we have many less disgusting expedients of which we may avail ourselves in such cases, intoxication will probably never come into repute as an adjuvant in the hands of the surgeon. Notwithstanding, to administer ardent spirit for the purpose of producing relaxation or prostration, is certainly more rational and less difficult than to use it on account of its stimulant property. To be useful for the latter purpose, it must be frequently repeated *in exactly the right dose;* too little would be of no avail, too much would produce torpor or prostration, or even destroy life, without evincing any excitement whatever. For the former purpose, all that is requisite is to give a sufficient portion, and the designed object will be accomplished with the utmost certainty.

I am aware it is said, that in cases of prostration from any cause, no bad consequences can result from giving distilled spirit till some action is produced—that, if one portion fail, two, three, or more portions should speedily be given; for as soon as the exciting agent is sufficiently power-

and we know what is done with it then. (*Hear, hear*). Besides, here is the *kun* or *koora*, a rank poison, which, when ottah is dear, is mixed with ottah, and eaten by the poor. Dr. Johnson next refers to Ireland. Why there was a great famine in that country, to relieve which the people of Liverpool sent cargoes of rice over as gifts, as I read in the papers, and disease naturally followed. Then, as to cholera in the Highlands, when Dr. Johnson says that rice is never seen there, I have only to say that a Highlander once told me that rice could not be the cause of cholera, because he was entirely fed when young upon rice, and yet never had the disease. In answer to what Dr. Johnson says about the banks of a river, I reply—(We did not here catch the Doctor's answer; but it was to the effect, that the supply of rice by boats on rivers might take place at the periods in which the disease ensued, in exact accordance with the progress of the boats, and yet be successive as regarded the intervals of its irruption.)

Dr. Johnson.—I am sure Dr. Tytler exonerates me in his heart from the charge of being so high an authority in India.

Dr. Tytler.—Justly or not, your name stands higher there than any other medical writer.

Dr. Johnson.—Well, I can only be gratified at the fact. As to the occurrence of cholera on one bank of the river soon after it has appeared all along on the other side only, and not simultaneously with it, it is incumbent on Dr. Tytler to show, in order to account for that, that boats laden with rice were, after going up one side, taken down the other, and had their cargoes sold to the inhabitants on shore. It also lies with Dr. Tytler to show, that a poisonous ottah is made by mixing *kun* therewith, of which we have no proof. Respecting the appearance of the disease amongst the military, also, how can Dr. Tytler account for the fact, that on the removal of the troops a few miles from the spot in which it began amongst them, the disease ceased? And pray how comes it that a gale of wind lately blew the cholera entirely away from London? Did it blow away rice and flour and all, at the same time?

The President.—I must relieve Dr. Tytler from the onus of accounting for any of the phenomena of the cholera in this country, because he distinctly said last evening that he could not pretend to account for the events of a disease which he had not yet seen.

Dr. Tytler.—As to the late gale producing the good effects ascribed to it, that is a mere assumption. I say it was *not* the gale. I went into the grocers' shops within these few days, and inquired the state of the rice sale, and I was told that nobody was eating it at this season of the year, but that it would probably come into use again about Christmas. Why, at Bancoorah the magistrate pursued an opposite course to that of Dr. Johnson, for in that place the magistrate actually ascribed the *production* of the cholera to a gale which occurred at that time, while Dr. Johnson claims for a gale the merit of having destroyed the disease. What can I say more?

Mr. Dendy.—I rise to object to some of the positions of Dr. Tytler as to the production of cholera in England by rice;—

The President.—I must again interfere.

Mr. Dendy.—Well, then, I content myself with saying, that I consider his documents to be of no *utility*, because they do not show us how to treat the disease in this country. I contend, also, that there is not an identity between the Asiatic disease and the disease here, on the ground that an almost undeviating symptom here is strangury, while in India Dr.

Tytler says that was not a symptom of the disease. I consider, too, that Dr. Tytler's communication is defective on this point, that it lays down too decisive a rule as to the cause of the disease. He will have nothing to do with any other cause than rice. Now I have had cases not only in which no rice had previously been taken, but in which I actually gave rice, and yet the patients recovered. Besides, *any* diseased grain will produce morbid effects. He mentioned last evening the cases of a merchant and a count, who could not eat rice without experiencing dropsy. Now I consider that the circumstance was merely an instance of that kind of antipathy which is common to some people on eating particular food. I know a lady who cannot swallow five strawberries without experiencing urticaria. Then as to the cessation of the disease by stopping the consumption of rice, why a mere change of wind will often check it. I attended a family in Knight's-court in which were two cases of cholera so far gone that I had not the slightest idea of their living through the night; but the wind changed before morning, and next day one of the patients sat up and laughed at me in the bed. I am sure, in fact, that the deleterious cause is in the atmosphere. Allow me to quote the cases at Clapham, where the opening of a cess-pool produced symptoms in persons exposed to the malaria, which Dr. Latham has since said were precisely such as he afterwards saw in cholera patients. It was malaria *then* and it is malaria *now*, ingesta of various kinds being the exciting causes. Let me also refer to the observation of Dr. Prout relative to the extraordinary density of the air just before the irruption of the cholera here, and also to the circumstance noticed at Pott's vinegar manufactory, where just at the time that the disease appeared in London there was such a peculiar change in the air that the fermenting process was impeded, and the whole of the vinegar then making was obliged to be thrown away. I admire Dr. Tytler's science and his great perseverance, but I wish he had had better facts to ground his views on. We might as well say that because green-gages in some cases produced the cholera, the disease should be called 'prunus choleriferus,' as that it should be called 'morbus oryzeus' in consequence of anything observed by Dr. Tytler; or that it should be termed 'morbus testudinis,' because some aldermen happened to have the cholera after going to a city feast.

Dr. Tytler.—I wish that that gentleman had done me the honor of sticking to my facts instead of quoting so many of his own. I have nothing to do, as the President has said, with the ditch at Clapham, nor with the belly-aches of little boys who eat green plums. I only know, that in a district where ten thousand people died of the cholera, every man in the jail of that district, who refrained from eating rice, escaped having the disease.

Mr. Dendy.—Does Dr. Tytler believe that the disease in this country is the disease of Asia?

Dr. Tytler.—I cannot tell. I have heard of a disease produced from vinegar-vats, green-gages, and tortoises. That is not the disease I saw in India. The only object I have in laying these facts before the Society is, to prove to you what has been the cause of the disease *in India*. I have studied the cause nowhere else, and therefore cannot pretend to speak of it. I do not want to trace the cholera everywhere to rice. I only say, that rice has produced it in the East. But then, mind, I show you that the very poison which has produced it there—bad rice—is at this moment selling in London as food at three-half pence a pound. Who can resist the inference! I warn people of eating that food; and will any man in this Society rise after what he has heard, and after examining the trash I have laid on the table, and say that such stuff as those examples exhibit is fit for human food? If so, let him proclaim it. If not, let him join me in my endeavor to banish this poison from the markets of England. (*Applause from all parts of the room.*)

Mr. Proctor.—To my mind there is reason to believe that Dr. Tytler has fully proved his position. A great deal of interest, however, is taken from the facts, in consequence of Dr. Tytler's inability to say that the Asiatic and the English diseases are similar.

Dr. Tytler.—I will give you my opinion then, Sir, that they are the same disease.

Mr. Proctor here made some remarks relative to the safety with which rice-water was given by him to cholera patients to drink.

Mr. Clifton.—I rise to say how much I think the Society is indebted to Dr. Tytler for the luminous exhibition of facts he has made to us. He has opened a new light in this country. We have had many discussions here and elsewhere on the subject of the cholera, but no hint has ever before been dropped with respect to its production by rice, nor has any intimation ever been made that such views as those of Dr. Tytler had at any time been entertained or promulgated by him. We are, therefore, hardly able at present to form a judgment on the subject. His facts relative to the jails at Jessore and Allahabad, and the Sepoys, are so strong, that if we are to believe what he says at all, there can be no doubt as to the cause of the cholera in India. I am myself disposed to say, from past impressions, that any deleterious food will produce the disease, under certain circumstances; but I repeat that I think we are deeply indebted to the author of the statements for the proofs he has afforded us of one most extensive cause of the disease. How important would it be could we cut off such a source of death, for I challenge the whole medical profession to name any remedy which will act as a certain cure for the disease. With regard to the rice, we ought to bear in mind that Dr. Tytler ascribes injurious effects only to *bad* rice, (*hear*, *hear*,) and Mr. Proctor should bear in mind that the rice-water given in cases here was probably made from the best rice.

Dr. Tytler.—Certainly. I allude merely to the diseased grain.

Dr. Whiting.—Nothing should stand in the way of a full prosecution of this inquiry. It is a most important one, and must be thoroughly probed. (*Hear, hear.*)

The usual hour of adjournment having passed, the request was made to Dr. T. that he would again meet the Society on Monday next, at eight o'clock. The wish was instantly acceded to, and the assembly departed."

Letter from Capt. Bowie, referred to in the Speech of Dr. Tytler, p. 335.

" To Dr. Tytler. " Batavia, Oct. 11, 1823.

" Sir,—Your letters in the Bengal papers some time ago attracted much notice, but I am well convinced not so much attention was paid them as they really deserved. What you mention of rice of a twelve month's growth back being prejudicial to health, if used as food, is beyond all doubt correct, as I and all my crew, 39 in number, have experienced; and I am further, beyond all doubt, convinced that it is still more pernicious when enclosed in a store, or ship's hold, where there is no circulation of air.

" On the 11th of July 1823 I landed at Passerwan, island of Java, a cargo of about 200 tons of rice that had been confined in a store belonging to the government for a space of about eighteen months. When taken on board it had all the appearance of good rice, and was sold as such, but its pernicious qualities were soon evinced.

"As soon as the vessel was loaded I repaired on board, and was immediately attacked, owing to my using this rice, with the disease called cholera morbus. My chief officer, Mr. Burn, and all my crew, were attacked a few days previous, and only saved their lives by remaining on deck. After the crew recovered, when sent below on ship's duty, if for the space of an hour, they had an immediate attack, and with much difficulty, through medicine, were restored.

"When at Blivous, island of Banka, a gentleman visited me, and after looking through the vessel below, although I informed him of the effects that were liable to ensue, and after eating of the rice, he went on shore complaining of an attack of cholera, and *died the next day*. He was the son of General Van Gheen, the commander of the cavalry at Batavia.

"I therefore, in support of your opinions, as I have read them in the Calcutta papers, do declare that I am positively convinced of the truth of what you have urged, although people have differed in opinion from you.

"I am, Sir, your most obedient servant,

"J. Bowie, commander of the brig *Elizabeth*."

The London Lancet, from which the foregoing account is taken, adds the following.

"Since writing out the above report, the gentleman who has been occupied on the task has had the following facts communicated to him, and evidence of their truth can, he believes, be furnished from competent sources. Fourteen or fifteen years since, a disease prevailed, almost universally, amongst the boys of the *London Orphan Asylum*, then situated in Hackney-road, the chief symptom of which was diarrhœa. The medical men who attended the institution were consulted, but no cause for the disorder could be discovered, and very great difficulty was experienced in its treatment. A principal article of food in the diet of the boys was rice of a very inferior quality, and at last, at the suggestion of the master of the school, the use of a very large share of the rice was discontinued in the meals, when the diarrhœa was checked, and the boys speedily recovered their health."

Whole number of deaths in Boston for the week ending January 4, 30. Males, 16—Females, 14.

Of convulsions, 1—unknown, 1—cramp in the stomach, 1—croup, 2—consumption, 1—quinsy, 1—lung fever, 4—dropsy on the brain, 2—influenza, 2—wounds, 1—disease of the heart, 1—fits, 1—scarlet fever, 1—hooping cough, 1—palsy, 1—inflammation of the lungs, 1—infantile, 1—measles, 1—teething, 1—old age, 1—inflammation of the bowels, 1—intemperance, 1. Stillborn, 1.

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